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CONTENTS

Contradiction

DONALD F. HENZE

Comment upon Professor Henze's Criticism

C. A. CAMPBELL

Can a Man Act upon a Proposition which he Believes to be False?

PAUL KASHAP

A Reply to "Projection and Paraphrase in Semantics"

JERROLD J. KATZ

A Note on Telepathy

PETER SWIGGART

Dispositional Properties

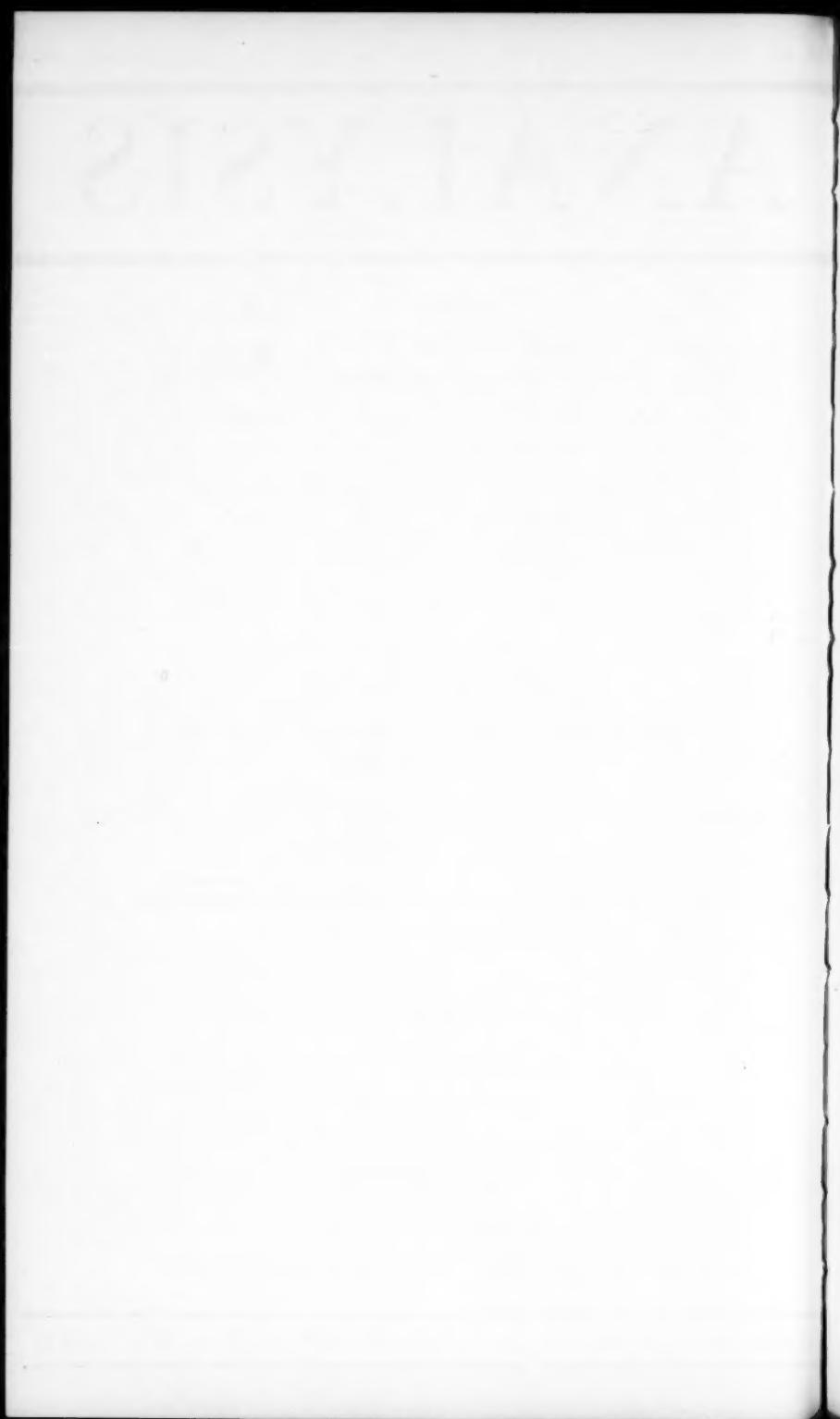
J. J. C. SMART

Red and Green All Over Again

JOHN HILTON

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CONTRADICTION

By DONALD F. HENZE

PROFESSOR C. A. CAMPBELL has raised anew an old question in a recent article entitled 'Contradiction: "Law" or "Convention"?' (ANALYSIS 18.4 (March 1958), pp. 73-6). His own answer is that it is a law. Against 'an important minority' of conventionalists, among whom he counts Professor A. J. Ayer, Campbell directs the following argument:

(1) '... discourse must be *intelligible* discourse.' (2) Hence any linguistic conventions we adopt must achieve this end. (3) If we adopt a convention such as 'p does not exclude not-p', we fail to achieve intelligibility (significance, meaningfulness). (4) Therefore 'we are obliged to accept the rule "p excludes not-p"'. (5) Consequently the rule 'is not a *convention*'. (6a) Moreover, such a rule is necessary and not merely convenient for intelligible discourse, 'because we are just not able to think save in terms of it'. (6b) This is 'equivalent to saying that it is a "law of thought"'.

This argument is vulnerable at several points. Its major flaw is that premiss (1) begs the question. It is difficult to escape the conclusion that Campbell is using 'intelligible discourse' in the sense of 'discourse free from contradiction' or 'discourse which abides by the rule of contradiction'. (Cf. (3) which states a *sine qua non* of intelligibility.) Now the contention that intelligible discourse must meet such a condition may well be true, but instead of proving that this must be so, Campbell's argument depends on its being so.¹

However, even if one were to grant that intelligible discourse is discourse free from contradiction, it would not follow that *all* discourse *must* be intelligible. '(The) standard purpose of speech, the intention to communicate something' is frustrated by self-contradiction, but to create frustration or puzzlement or even to exercise the vocal chords *may* be a purpose of language; so that contradicting oneself may not be pointless at all.² Against premise (2), then, it may be argued that discourse need not be fashioned to achieve intelligibility.

The transition from (4) to (5) is also open to question. This portion of Campbell's argument may be re-stated as follows: If we are *obliged*

¹ The significance of the 'must' which occurs in statements like (1) was questioned long ago by Ayer himself: Is this an affirmation of the rule of contradiction? See Ayer's contribution to the symposium 'Truth By Convention', ANALYSIS 4.2 and 3 (Dec. 1936), p. 22.

² P. F. Strawson, *Introduction to Logical Theory* (London, 1952), pp. 2-3. Cf. Karl Britton, *Communication* (London, 1939), p. 257: 'Contradiction and verbal nonsense also have their uses in metaphysic and in poetry. Contradiction is itself a kind of maximal case of ambiguity.'

to accept the rule of contradiction, then it is not a *convention* ((4) and (5)). We are obliged to accept it (from (1), (2), and (3)). Therefore it is not a convention.

Although this argument is valid, a premiss is not true: Campbell has overstated his case by taking (5) to be a logical consequence of (4). The initial premiss of this sub-argument is equivalent to the assertion that if the rule of contradiction is a convention, then we are not obliged to accept it. And this need not be true. 'Convention' is defined by the *Shorter O. E. D.* as 'general agreement or consent, as embodied in any accepted usage, standard, etc.'. In view of this definition, it makes perfectly good sense to regard *accepted usage* or *general agreement or consent* as grounds for obligation. Thus, classifying a rule as a convention does not in itself exclude it from being obligatory.

Campbell's defence of a more or less traditional interpretation of contradiction fails, but conventionalism, of course, is not automatically installed as the proper view. As a matter of fact, Campbell is able to pose some rather embarrassing questions for Ayer's brand of conventionalism. The most difficult challenge a conventionalist must meet is the demand that he suggest a different convention from our own, i.e., one which does *not* include the adoption of the rule 'p excludes not-p'. This, in turn, has the effect of forcing the conventionalist to explain what he would mean by 'p and not-p'. It will not do, as Campbell points out against Ayer, to offer a new use for the word 'not', because the issue is one which concerns the possibility of altering one's logic, not the English language. For example, 'the paper is both white and not-white (all over at the same time)' might be the conventional way of asserting that the paper is grey (as Ayer suggested in his B.B.C. debate with Father Copleston, reprinted in *A Modern Introduction to Philosophy* (Glencoe, Ill., 1957), ed. by P. Edwards and A. Pap, pp. 598-9); but this mode of speaking would not be counter to the *logical* rule of contradiction. This, as I understand it, is Campbell's complaint against Ayer, and I think he (Campbell) is right in pressing the charge.

There is another stock argument against the conventionalist interpretation of logical rules: If these rules are conventions only, then why does it seem that we cannot alter them? This, too, calls for a stronger rejoinder than the one made by Ayer against Father Copleston in their debate.

Now I suspect that it is the force of these counter-arguments against conventionalism, rather than any arguments for the traditional views, which gives Campbell's position its greatest plausibility. But rather than review the disputes among those who regard contradiction as a law of thought, a law of things, or a logical convention,¹ I should like

¹ A classification of these different views concerning contradiction, together with a brief summary of pros and cons, may be found in K. R. Popper's contribution to the symposium 'Why are the Calculuses of Logic and Arithmetic Applicable to Reality?' *Proc. Arist. Soc.*, Supp. Vol. 20 (1946), pp. 48-51.

to discuss a basic feature of this problem which is generally neglected. I refer to this unresolved question: Is contradiction most properly to be called a principle, a law, or a rule; or is some other classification more appropriate? A sampling of usages indicates an unpatterned shift from one term to another and also a liberal sprinkling of double quotes to warn the reader against taking too literally the term which is employed. Campbell himself slips into both practices. "Law" appears in the title of his article while "law", 'law', and 'rule' occur in the body of the article. There is a measure of unclarity in this diversified usage which should not be overlooked. Doing so, I fear, has given rise to a number of unnecessary disputes.

Contradiction is spoken of most frequently, perhaps, as a *law*. But what sort of law? Like a law of state (prescriptive) or a law of nature (descriptive)? Neither analogy is helpful. The first alternative can be met by the second of the traditionalists' stock arguments (see above), the second alternative by simply pointing out that people *do*, on occasion, contradict themselves. Yet, there is another meaning of 'law' which, if applied to explicating the phrase 'law of contradiction', escapes both these objections. This is 'law' in the sense of a custom or habit.¹ The rationale behind such a logical custom, as Strawson and Britton have pointed out, is communication. If we regard an assertion about the law of contradiction as an utterance *reinforcing* this particular use of language, namely the communicative, then we can avoid the difficulties stemming from both the prescriptive and descriptive senses of 'law'. 'Law' in this third sense is *subscriptive*: used to impose a constraint on our linguistic practice (though not by the fiat of some external authority—rather we in society impose it upon ourselves), and used to acknowledge one's concurrence with general practice (though not merely a general statement about that practice).

Closely related to contradiction as law is the concept of contradiction as a *rule*: 'regulating the procedure or method necessary to be observed in the pursuit or study of some art or science'. But whereas the uniformity or regularity of self-consistent usage is stressed by talking about the law of contradiction, contradiction as a rule seems to open the possibility of *exceptions*—for it is true that rules admit of being broken. It has been said that if we break this or that game rule, then we are no longer playing *that* game. But what of language? If a man contradicts himself does he cease playing the language-game? No, because there is not *the* language-game, only *many* language-games. Contradiction frustrates the game of communication, but one may not always be playing *that* game. Campbell talks as if we have no choice in this matter, but I think he construes language much too narrowly.

The notion of contradiction as a *principle* seems most appropriate

¹ The source of this and subsequent definitions is the *Shorter O. E. D.*

for those contexts in which contradiction serves as the underpinning for a rather definite set of propositions, or as a guide for the construction of such a set. The development of natural languages is seldom the result of conscious, deliberate planning; hence, contradiction as a principle is better illustrated by the property of consistency (non-contradiction) demanded of formal deductive systems. But it should not be confused with contradiction as a *theorem*, i.e. the last statement of a proof. (See, e.g., *3.24 in *Principia Mathematica*: ' $\vdash . \sim(p . \sim p)$ ' is 'the law of contradiction', say Russell and Whitehead, who add that 'in spite of its fame we have found few occasions for its use' (2nd edn., p. 111).)

Two recommendations emerge from the foregoing distinctions. Firstly, any discussion of contradiction should be marked by a decision as to whether contradiction is being considered in the context of an ideal or artificial language or in the context of some natural language or languages. Secondly, if the former, is it one of the properties of the devised language (or system) or is it a theorem *in* the system (given that the system is deductive)? If the latter, then its status as a rule will depend on our language-game, its status as a law on how accustomed we are to playing the language-game of communication. Adhering to some sort of scheme as this should limit, if not eliminate, disputes about contradiction.

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COMMENT UPON PROFESSOR HENZE'S CRITICISM

By C. A. CAMPBELL

IN the introductory paragraph of my article I gave what I described as the 'gist' of the case against the conventionalist interpretation of the principle of contradiction. It is to the argument there sketched that Professor Henze's analysis and criticism relate. I shall take up each of his criticisms in turn.

1. 'Its major flaw', Henze says of the argument, 'is that premiss (1) begs the question'. According to him, I am using 'intelligible discourse' in the sense of 'discourse free from contradiction', and thus assuming what is to be proved.

In fact I do not so use it; and I have difficulty in understanding why Henze should suppose that I do. The only ground he offers is his parenthetical reference to (3) in my argument, which, we are told, 'states a *sine qua non* of intelligibility'. But if the reader will look at the relevant

passage in the actual text of my article, he will find that it is by no means a matter of just 'stating':

.... But do we speak intelligibly—do we convey any determinate meaning even to ourselves—if we adopt a convention such as 'p does not exclude not-p'? If, for example, you say 'Ayer is a philosopher', and this proposition is, in accordance with your convention, compatible with 'Ayer is not a philosopher', is it not obvious beyond argument that what you are saying has no significance whatever?¹

More formally, the argument is that since all discourse that is intelligible has determinate meaning, and since no discourse that is self-contradictory has determinate meaning, therefore no discourse that is intelligible is discourse that is self-contradictory. Either or both of the grounds offered for the conclusion can of course be questioned. But my immediate point is just that grounds *are* offered, an argument *is* advanced. The charge of begging the question thus seems to be without foundation.

2. The next criticism is, as I understand it, to the following effect. Even if it be granted to me that *intelligible* discourse is discourse free from contradiction, it is an error to suppose that *all* discourse is *intelligible* discourse; and it is illegitimate therefore to conclude that for discourse *in general* 'any linguistic conventions we adopt must achieve this end' (of intelligibility, and consequently freedom from contradiction).

My answer must again be a reference to the text of my paper. I did not affirm (and I do not in fact hold) that all discourse is intelligible discourse. What I actually wrote was 'Granted that logic has to do with discourse, that discourse must be intelligible discourse'²—in other words, the discourse with which *logic* has to do is intelligible discourse; and I went on, in the same vein, 'Hence any speech conventions which a specific logic prescribes must be such that if we speak in terms of them we speak intelligibly'.

Henze may disagree with the view that the only statements with which *logic* has to do are statements which carry a determinate meaning and are in that sense 'intelligible'. What he is here criticising, however, is not this view, but a different one which is not to be found in my paper.

3. Henze's final criticism concerns the transition from (4) to (5) in my argument (as he has summarised it). The premiss of this sub-argument, he points out, is equivalent to the assertion that if the rule is a convention we are not *obliged* to accept it. He argues, however, on the basis of the definition of 'convention' which he quotes from the *Shorter O. E. D.*, that a rule may be a 'convention' and yet be 'obligatory'. For 'it makes perfectly good sense to regard accepted usage or general agreement or consent as grounds for obligation'.

¹ ANALYSIS, March 1958, pp. 73-4.

² *Lor. cit.*, p. 73.

That these do constitute grounds for obligation up to a point, I do not dispute. If only in the interest of effective communication with others, we most of us recognise that we should not without special reason violate rules which have the 'authority' of accepted usage. But it is not in this *weak* sense of 'oblige' that the term is being used at (4) in my argument. 'Obliged' there, as the context indicates, is being used in its strictest, 'absolute' sense—we are 'obliged' to accept the rule '*p* excludes *not-p*' because '*we can no other*'. The rule (I argue) is for intelligible discourse a *necessary* rule, since only in terms of it is it possible to think at all. Now when it is appreciated that 'obliged' is being used in its absolute sense, it seems to me that the validity of the transition from (4) to (5) is hardly open to question. For it is of the essence of a rule instituted by 'convention' that a *different* rule could have been substituted. There are thinkable alternatives to it; and although the very fact that *this* is the rule which has been made the convention carries with it some obligation to accept *it*, it is always possible, though it is often foolish, to prefer one of these alternatives. On the other hand, it is of the essence of a rule that we are obliged to accept, in the absolute sense of 'obliged' used in (4), that there are *no* thinkable alternatives. It is a rule which we cannot *not* accept, an inherently necessary rule. It is therefore a rule which 'is not a convention'.

The rest of Henze's paper raises some interesting questions in a more general reference. Comment upon these must obviously be rationed, but I may perhaps be permitted a very few words on a point which seems to me to merit deeper exploration than it commonly receives. Asking what kind of a law 'contradiction' is, if a law at all, Henze dismisses the suggestion that it is a 'descriptive' law by 'simply pointing out that people *do*, on occasion, contradict themselves'.

In my opinion this way of settling the matter is altogether too easy. No one wants to deny, of course, that people do, on occasion, 'contradict themselves' in *some sense*. But in just *what* sense? Is there really good evidence that anyone ever intellectually asserts the equivalent of 'S both is, and is not, P at the same time and in the same relation'? I very much doubt it. There has been, I suspect, far too little analysis of what is actually being thought in ostensible cases of 'self-contradictory thinking'. For my own part, I have found that what analysis of such cases reveals is always contradiction between distinct acts of thought in a single argument or train of thought—never a single act of thinking S as both being and not being P. But for samples of the sort of analyses I have in mind I fear I must refer the reader, if he should be interested, to a more detailed treatment of this topic in another place.¹

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¹ *On Selfhood and Godhood*, pp. 384-7.

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CAN A MAN ACT UPON A PROPOSITION WHICH HE BELIEVES TO BE FALSE?

By PAUL KASHAP

SUPPOSE that before a man boards a plane he takes out a policy insuring himself against death in a crash. Now it may be said that the proposition upon which he acts in taking out the insurance is that he will die in the plane crash, and this he believes to be false. The evidence of his belief in the falsity of the proposition consists plainly in his boarding the plane, since if he believed that the plane would crash and that he would die in it, then (assuming that he is a normal person) he would not travel on that plane.

Another example: A man's boat sinks ten miles off the nearest coast and he starts swimming toward the shore. Let us assume that the man knows that there is in fact no possibility of anyone coming to his aid. Now this man, in swimming towards the shore, is said to be acting upon the proposition that he will reach the shore, and yet he believes this proposition to be false, because from past experience he knows that he cannot swim more than a mile.

I propose to argue in this paper that a man can *not* be said to act upon a proposition which he believes to be false. I do so by showing that in the above two examples the proposition which the man *acts* upon is not the *same* proposition which he believes to be false (and that therefore it does not seem possible to replace the proposition which the man acts upon with the proposition which he believes to be false).

It will be noted that in the above two examples, the propositions which the men are said to act upon, and which they believe to be false are, respectively, (1) 'I shall die in the plane crash', and (2) 'I shall reach the shore'.

Professor Chisholm has outlined four senses of the phrase '*S acts upon* the proposition *b*'¹, and he assumes that at least one of them fits the above examples. I wish to examine these four definitions and see if any of them does fulfil the claim.

I. Professor Chisholm argues that '*S acts upon b*' can be explicated as '*S provides for the possibility of b being true*', and this latter, in turn, as '*S acts in order that, if b should be true, a certain end would then be realized*'.

Now it must be remembered that the statement in question is '*S acts upon the proposition b which he believes to be false*'; *b* stands for

¹ R. M. Chisholm, 'What is it to act upon a proposition?', ANALYSIS, Oct. 1961.

'he will die in the plane crash'. If we replace *b* with its translation we get, 'S acts upon the proposition that he will die in the plane crash'. Now according to Chisholm's explication of 'S acts upon *b*', the above statement is to read: 'S acts (i.e. insures himself) in order that, if it should be true that he dies in the plane crash, a certain end might then be realized (i.e. his family will then be provided for)'. This explication, it must be admitted, makes perfect sense of the notion of 'acting upon'. The difficulty arises when we try to substitute the above translation of 'S acts upon the proposition that he will die in the plane crash' in the compound statement, 'S acts upon a proposition *b*, which he believes to be false'. If we do, the following is what we get: 'S insures himself (acts) in order that, if it should be true that he dies in the plane crash, his family will then be provided for; which he believes to be false'.

If Chisholm is right in interpreting 'S acts upon the proposition *b*' in this way, then it should be possible to add, at the end, 'which he believes to be false', and yet be able to make sense of the whole statement. But it seems extremely difficult, if not impossible, to understand in the above statement, *what* he believes to be false. That he insures himself? No. That he might die in the plane crash? No. That, if he dies, his family will be provided for? No. In fact he does not believe anything in it to be false at all.

Chisholm gives the following example: 'The cautious scholar who checks his references before writing "Megitius was condemned by the provincial Synod of Seville in 782" is providing for the possibility that his statement is incorrect; thus he may be said to be acting on the proposition that Megitius was *not* so condemned.' I wish to contend that there is something wrong with Chisholm's argument. It is suggested that our scholar is acting upon a proposition which he believes to be false. But if the scholar, in checking the reference, is acting upon the proposition that Megitius was *not* so condemned, then surely he is acting upon the proposition that Megitius was *not* condemned by the Synod of Seville in 782, which proposition he must believe to be true. One may further argue that when the scholar checks the reference, he is not absolutely certain that it is *true* or that it is *false*. In looking up the reference he must be non-committal with respect to the truth-value of *b*, and his action should not be taken to imply that *b* is false or that *b* is true. Hence he cannot be said to be acting upon the proposition that Megitius was *not* so condemned. But in providing for the possibility of *b* being true, he does act upon a proposition which he believes to be true, in so far as 'there is a possibility of *b* being true' he believes to be true. That is to say, so far as the man, in acting upon *b*, believes that the possibility of *b* being true is a genuine and relevant possibility, he acts upon a proposition which he believes to be true, and not upon one which he believes to be false.

II. According to the second definition, 'S acts upon the proposition *b*' means 'S provides for the fact that *b* is true; i.e. S acts in order that, since *b* is true, a certain end may thus be realized'.

According to this definition, 'the man acts upon the proposition "he will die in the plane crash"' means 'the man provides for the fact that "he will die in the plane crash" is true'. Here (apart from the term 'fact' being rather misleading), 'the man, in insuring himself, provides for the fact that he will die' entails that the man (in some sense) knows that he *will* die. (He may have planted a bomb on the plane.) Therefore, if 'S provides for the fact that he *will* die', it does not seem to make sense to add that 'he believes this to be false'. If a man knows that he has planted a bomb on the plane to go off in mid-air, and that if he flies in that plane he will die, then it is irrational to add that he believes that it is false that he will die. Thus this interpretation of 'acts upon the proposition' does not seem to be compatible with the sense of 'act upon' in the above examples.

III. In the third sense, according to Chisholm, 'S acts upon the proposition *b*' means 'S relies upon *b* being true; i.e. if (what is contrary to fact) S did not believe *b*, then S would be providing for the possibility of not-*b* being true'.

According to this definition, the man, in taking out the insurance, relies upon the proposition 'he will die in the plane crash' being true; i.e. if (what is contrary to fact) the man did not believe that he will die, then he would be providing for the possibility of his not dying in the plane crash being true. That is to say, one of the 'provisions' would presumably be *not to rely* on the proposition that he will die in the plane crash. But since this provision is not made by him (because he takes out the insurance), he must rely on the truth of the proposition that he will die. It seems logically odd to speak of *relying* on the proposition that I shall die in the plane crash, and then to add 'but I believe it to be false'.

When S relies upon *b* being true, his action has a relation of necessary dependence on the proposition which he relies upon, such that his action would be different if he did *not* rely on *b* being true. And this is precisely what Chisholm means by the explanatory clause to Definition III. But one cannot act in a way which exhibits *reliance* on the truth of the proposition one is acting upon, and *believe* that proposition to be false. The logic of the word 'reliance' is contrary to that of 'absence of belief', and it would be misleading to use them as if one corroborated the other. Hence I conclude again that this definition is inadequate.¹

¹ There is another way in which the inadequacy of this definition can be demonstrated—by showing that the definition leads to a contradiction. I am indebted to Mr. James K. Mishalani for the following argument.

(1) S acts upon *b* and S believes that *b* is false.

continued overleaf

IV. Fourthly, 'S acts upon the proposition *b*' means 'S acts in order thereby to act as he believes he would act if he were providing for the possibility that *b* is true'.

This definition, when applied to our example, yields 'The man insures himself in order thereby to provide for his family as he believes he would have done if he were providing for the possibility of his dying in the plane crash'.

Now the point to be remembered here is not whether the above statement by itself makes sense, but whether the above statement as an explication of 'S acts upon *b*' makes sense when we replace it in the statement 'S acts upon the proposition *b*, which he believes to be false'. And it seems that it does not. For the man, in insuring himself against death in a plane crash, is providing for the possibility of his dying in the plane crash. And in making this provision, he is acting upon the proposition that it is possible that he may die in a plane crash, which he must (logically must) believe to be true, and not false.

It seems, therefore, that none of the four senses of 'acts upon the proposition' given by Professor Chisholm fits the case in which a man 'acts upon a proposition which he believes to be false'. (The arguments I have used can be applied to either of the above examples.) Now the question is, what is the sense of 'act' as used in the case in question? And furthermore, in what sense can a man be said to 'act' upon a proposition which he believes to be false?

In order to answer this question, it will be necessary to analyse the term 'action'. An adequate analysis of this difficult concept is beyond the scope of this paper. I shall only attempt to give some hints as to the direction in which it may be fruitful to proceed.

Suppose I ask a person (imperatively), 'Act upon the proposition "You are going out of the room"'. And, by way of compliance, he goes out of the room. Now I shout back at him to ask, 'Did you believe the proposition on which you acted (just now) to be false?'. I presume he would reply in the negative. 'But wait', he says, 'tell me to do the same thing again'. When I repeat my first request, he calmly saunters into the room and declares, 'Now I have acted on the proposition which I believe to be false'.

But this person has obviously made some mistake. The point is, in acting contrary to the proposition, has he acted upon *that* proposition

- (2) From this it follows (simplification) that S acts upon *b*.
- (3) If S did not believe *b* then S would be providing for the possibility of not-*b* being true. (Chisholm's Definition III.)
- (4) The antecedent of a counterfactual being always false, it follows from (3) above that S believes that *b* is true.
- (5) S believes that *b* is false (follows from (1) by simplification).
- (6) From (4) and (5), by conjunction, we get, S believes *b* is true and he believes that *b* is false—which is a contradiction.

which he claims to believe to be false? And the answer is, clearly no. If he did think that he was acting upon that proposition, then either he has failed to understand what constitutes 'acting upon a proposition', or he has *acted as if* he were acting upon the proposition upon which he was not acting. This may even be an instance of 'play-acting'.

Now although to 'act as if' or to 'play-act' *is* to act in some sense, it is a different sort of activity from the one in which a man acts upon a proposition which he believes to be true. To lump them all together as 'acting' is to fail to take account of the logical differences in the various senses of the term 'acting'. It is perhaps difficult and yet important to recognize, for example, that an actor's performance on the stage is, as a rule, 'play-acting', but it may also be 'acting'. As 'play-acting', he can play-act upon a proposition which he *believes* to be false. (I italicise 'believe' to differentiate it from 'make believe', or 'pretend to believe'.) But while he is 'play-acting', he may also be 'acting' on the proposition which he believes to be true. For example, an actor may be playing the part of Richard II and thus 'play-acting' upon the proposition that he is Richard II, which he *believes* to be false. But while he is play-acting as Richard II, he may also be 'acting' on the proposition that he is play-acting as Richard II, which he believes to be true.

It seems, however, that a man cannot be said to be 'acting' and 'play-acting' upon the *same* proposition. One can be said to 'act' only upon a proposition which one believes to be true. And 'acting upon a proposition which one believes to be true' is the distinguishing characteristic of acting as opposed to play-acting or 'acting as if'.

In the example of the swimmer, he is said to be acting upon the proposition 'he will swim ashore', which he believes to be false. His intention clearly is to make the shore. Now it will be difficult to account for his action if it is explained in terms of his belief that he will *not* make the shore. More accurately, it may be said that he believes that *the probabilities are* that he will not make it (on the evidence of his past performances). And this is all that he can legitimately be said to believe, if his action of swimming toward the shore is to be explained. Thus, if he believes that the probabilities are that he will not make the shore he cannot be said to be 'acting upon the proposition "he will make the shore"', which he believes to be false'. The proposition which he acts upon must include *some* element (however slight) of the probability of his making the shore, despite inductive evidence from past performances. Therefore, if he believes that the probabilities are that he will not make the shore, then the factor of doubt or uncertainty necessarily implied in a probability-statement must be included in order to explain his action in swimming toward the shore. And this can be put in the form of a proposition 'there is a (one in a million, or whatever)

chance that he will make the shore'. He may believe the probabilities to be low, but since it is in principle impossible to say (in this case) that there is *no* probability at all, he must, in swimming toward the shore, act upon the proposition 'there is probability' that he will make the shore'. And this, I suggest, he must believe to be true. Hence I conclude that it is false to assert that a man can act on a proposition which he believes to be false.

Brown University

A REPLY TO "PROJECTION AND PARAPHRASE IN SEMANTICS"

By JERROLD J. KATZ

IN a recent issue of ANALYSIS,¹ Mr. Fodor proposed the thesis that under certain conditions grammatical transformations preserve paraphrase relations. In particular, he claimed:

- (1) If two kernel sentences k_1 and k_2 are P-related and if the transformations T_1, T_2, \dots, T_n are applied to k_1 and k_2 in the same order (i.e. T_1 is applied to k_1 resulting in s_1^1 , then T_2 is applied to s_1^1 resulting in s_1^2 , . . . , then, finally, T_n is applied to s_1^{n-1} resulting in s_1^n), then the two final results s_1^n and s_2^n are P-related.
- (2) If two P-related sentence fragments are embedded in two P-related matrix sentences by the same transformation, then the resulting sentences are P-related.

If these claims are true, or even true in general, Semantics benefits by a theory for projecting paraphrase relations among kernels to paraphrase relations among the indefinitely many sentences constructed from them transformationally.² Not only would this effect a systematization of the P-relations in the language, but it would explain one facet of the speaker's

¹ ANALYSIS, 21.4 (March 1961), pp. 73-77. My reply requires no more of a knowledge of transformational analysis than Mr. Fodor's original paper.

² It should be made clear that, contrary to Mr. Fodor's remark (p. 77), his two conditions cannot be construed as restrictions on putative transformations, because no putative transformations which prove themselves in the grammatical analysis of English (which generate the sentences required without producing non-sentences, which explain and predict ambiguities, and which explicate relations between sentence types) will be rejected because they conflict with a semantic theory. Rather, the test is the other way round. This is my reason for referring to Mr. Fodor's two conditions as 'a theory for projecting paraphrase relations' instead of 'restrictions on putative transformations'.

linguistic ability, i.e. his ability to recognize and freely produce paraphrases of a sentence. Decisions about paraphrase relations exhibit a high degree of systematicity: fluent speakers do not require special training for new sentences to recognize a great many of their P-relations to other sentences.¹ This strongly suggests some underlying mechanism whose reconstruction in the form of explicit rules of a projection theory would serve to explain the speaker's paraphrasing skills.²

In this paper, I will argue that Mr. Fodor's projection theory is entirely inadequate. I will show that his explanation in terms of (1) and (2) fails to apply to the theoretically interesting cases, the cases where an explanation is most needed and where one would provide most insight, and that it applies to the theoretically uninteresting cases only vacuously so that it offers no explanation even for them. Furthermore, I will argue that his claim that understanding a sentence at least in part involves the ability to recognize its paraphrases is dubious, and that, rather than offering insight into the notion of understanding a sentence, his contention assumes a measure of insight we do not have.

It is by no means clear what counts as a P-relation. Mr. Fodor does not intend them to preserve truth. He says that two sentences can be P-related even though one might be true while the other is false.³ This is the reason he claims that paraphrase is not synonymy. Further, his examples show that he admits paraphrase relations between questions, where no question of truth preservation is relevant. This seems quite counter-intuitive because one would not say that a sentence S_1 is a paraphrase of another sentence S_2 if S_2 is true and S_1 is false. Whether or not this intuition proves correct, the notion of a P-relation is definitely in need of clarification, because as it now stands we cannot determine when a P-relation holds between a pair of kernels, and, without being able to determine this, Mr. Fodor's projection theory cannot even get started. One further point: if my criticism that his account does not help to explain how sentences are understood is well taken, it is no longer clear of what relevance a notion of paraphrase which fails to preserve truth can be for Semantics.

Nonetheless, we can put some qualms aside and guide ourselves by clear cases. P-related sentences fall into two groups. First, there are what I shall call 'E-P-related' cases: pairs of sentences that are P-related by virtue of P-related expressions appearing at identical places in other-

¹ On page 74 of Mr. Fodor's paper he asserts that, with a knowledge of the P-relations among some proper sub-set of the sentences of the language, a speaker can determine the P-relations among all other sentences in a mechanical fashion. I think that to say this can be done in a mechanical fashion is overstating the case. When sentences become extremely complicated or speakers become scrupulous about the notion of a paraphrase, some cases cannot be decided until some new knowledge about the language is acquired.

² This way of explaining a linguistic ability is quite common in modern approaches to grammar. Cf. N. Chomsky, 'Explanatory Models in Linguistics' (mimeographed, Massachusetts Institute of Technology, 1961).

³ Footnote 1, p. 74 of Mr. Fodor's paper.

wise equivalent sentences. In E-P-related cases, we have, as a rule, two sentences with essentially the same grammatical structure. The notion of P-related expressions may be left open. We may give it some content by thinking of synonymous expressions, nearly synonymous expressions, theory-determined equivalences, stipulative equivalences, etc. Examples of some typical E-P-related sentences are: 'The man plays the game' / 'The σ plays the game'; 'The game is played by the man' / 'The game is played by the σ '; 'Does the man play the game?' / 'Does the σ play the game?'; 'What does the man play?' / 'What does the σ play?'; 'The man does not play the game' / 'The σ does not play the game'; 'The man's playing of the game. . . .' / 'The σ 's playing of the game. . . .'; etc. In each case ' σ ' is to be some expression P-related to 'man', e.g. 'rational animal', 'featherless biped', or whatever you wish. Second, there are what I shall call 'G-P-related' cases: pairs of sentences that are P-related by virtue of different but appropriate grammatical structures. Basically, such cases do not involve P-related expressions.¹ Examples of some typical G-P-related cases are pairs of sentences from the following list: 'I saw the man'; 'The man was seen by me'; 'It was the man that I saw'; 'The man was whom I saw'; 'The one whom I saw was the man'; 'The one seen by me was the man'; and so forth.

G-P-related cases are the theoretically interesting ones because they are actually where transformations produce P-relations,² and, conversely, E-P-related cases are the theoretically uninteresting ones because transformations do not account for the speaker's ability to make decisions about their paraphrase relations. That Mr. Fodor's projection theory does not apply to G-P-related cases will be established below. Here we wish to show that though (1) and (2) apply to E-P-related cases, they apply only to the least philosophically interesting of these and to them vacuously.

Because Mr. Fodor's theory must start its projections from P-related kernel sentences, it is even inapplicable to many E-P-related cases, among which are virtually all the philosophically interesting ones. There are, of course, P-related single words in English, such as some of the so-called synonyms listed by a good dictionary, but philosophically interesting P-related expressions usually in one or both cases involve constructions of words where two or more play different grammatical roles. As a rule, to obtain an expression which is P-related to a particular term it is necessary to resort to constructions involving prenominal or post-nominal adjectivals, preposition plus noun phrase modifiers, adverbials,

¹ I say 'basically' because any pair of G-P-related sentences may be turned into another pair of P-related sentences by replacing an expression in one by an expression P-related to the corresponding expression in the other. But, clearly, such cases are derivative.

² In fact, Mr. Fodor himself makes this point when he says, concerning the pair 'She admired me' and 'I was admired by her', two of his examples, 'It is P-relations of this kind of which we particularly need an account'; p. 75.

strings of these, and mixed strings, all of which are built in a sentence by transformations so that the sentence is not a kernel. Hence, all E-P-related pairs in which there is a P-related expression constructed transformationally and which cannot be reduced to a P-related kernel pair (e.g. 'Bachelors enjoy life' and 'Unmarried males enjoy life' cannot be reduced to a pair of P-related kernels because 'Bachelors enjoy life' and 'Males enjoy life' are not P-related) are beyond the scope of Mr. Fodor's projection theory.

In E-P-related cases everything depends on the unanalyzed notion of P-related expressions. The so far unknown criteria for the application of this concept completely determine when two sentences are E-P-related. This can be seen from the following:

- (3) If there are two sentences S_1 and S_2 such that S_1 differs from S_2 only in that σ_1^1 appears in S_1 where σ_1^2 appears in S_2 , σ_2^1 appears in S_1 where σ_2^2 appears in S_2 , . . . , and σ_n^1 appears in S_1 where σ_n^2 appears in S_2 and σ_i^1 is P-related to σ_i^2 , then S_1 is E-P-related to S_2 .

This is trivially true, since the definition of 'P-related expression' must have (3) as a consequence. (3) makes it obvious that the notion of P-related expressions alone suffices to determine E-P-related sentences. But this concept is not available and there is not the slightest suggestion of how it might be analyzed. Therefore, Mr. Fodor's explanation is vacuous for these cases.

Mr. Fodor is certainly right that the ability to determine P-relations is an important linguistic ability and one that badly needs explanation. But his claim that the determination of P-relations is necessary to understanding a sentence is somewhat dubious. He says that to understand a sentence it is necessary to be able to judge what constitutes paraphrases of it as well as other speakers and that to understand a pair of sentences it is necessary to be able to determine whether they are P-related.¹ Yet a person might be said to understand a sentence even though he is unable to judge its paraphrases adequately. And a person might understand pairs of sentences even though he is incapable of deciding whether they are P-related or not. I think we would have to say both these things if someone displayed sufficient appropriate behaviour. A person might very well understand a sentence without, at the same time, recognizing a connection between it and other sentences, viz. the P-relatedness of certain expressions in each. I think there are persons whose mental equipment is not adequate to afford them the use of the concepts necessary to mark such connections, though they behave appropriately enough in general for us to say they understand sentences. I do not think that making paraphrase decisions adequately is a necessary condition of understanding, but I do think making such decisions and behaving

¹ Mr. Fodor's paper, p. 76.

appropriately are each tests whose positive results can be sufficient to warrant the conclusion that a sentence is understood. I may well be wrong, but here the notion of understanding a sentence needs analysis, without which Mr. Fodor's side on this issue must be somewhat dubious, as, of course, must mine.

With G-P-related cases, the question of whether or not two sentences are P-related depends uniquely on whether or not a certain transformation institutes a paraphrase relation between them. The general inapplicability of (1) and (2) to G-P-related cases can be seen from the fact that with (1) and (2) we start with P-related *kernel* pairs and project this P-relation to pairs of sentences by the *same* ordered set of transformations. With G-P-related cases, however, there are no P-relations among kernels and pairs of G-P-related sentences can be constructed from a pair of P-related sentences by *different* transformations. In the case of G-P-relations, generally, one or more sentences are operated on by a transformation, then the result can be operated on by a different transformation, and this process is repeatable usually with different transformations entering at different steps—leaving a trail of P-related derived sentences.

Moreover, the systematization of P-relations for G-P-related cases is by no means an easy or simple matter, because the same transformation sometimes produces derived sentences which are P-related to their source sentences and sometimes produces derived sentences which are not P-related to their sources. For example, the passive-transformation produces a source-derived sentence pair which is P-related in the case 'I saw the man' and 'The man was seen by me', but not in the case 'Everyone in the room speaks two languages' and 'Two languages are spoken in the room'.¹

Finally, let us briefly consider an extension of Mr. Fodor's theory which might seem to provide a way out of these difficulties. Instead of (1) we have:

- (4) If there are two sentences (not necessarily kernels) and if they are P-related, then if the transformation T is applied to both, the resulting sentences are P-related.
- (2) is already in suitable form. The claim that (2) and (4) are generally applicable to cases where G-P-related sentence pairs transform into G-P-related sentences is false because this theory applies to very few such cases. For example, the G-P-related pair 'I saw the man' and 'The man was seen by me' transform, respectively, into 'Whom did I see?' and 'Who was seen by me?' but *not by the same transformation*. This is because a transformation cannot operate on two sentences unless both have the same grammatical structure—the unique structure which defines the domain of the transformation. This restriction greatly limits

¹ N. Chomsky, *Syntactic Structures*, Mouton and Co., 'S-Gravenhage (1957), pp. 100-101.

the class of sentences on which a transformation can operate, and, in general, this narrowing down is such that only few transformations apply to both members of a G-P-related pair. For, being G-P-related pairs, the sentences are transformationally connected which means that structural changes on one have produced or led to the production of the other so that it must be structurally different in a significant respect.

Relaxing this restriction will not save the new theory because allowing transformations to operate on sentences with essentially different grammatical structure leads immediately to counter-examples.¹ For instance, the matrix sentence 'The man hit the man' can, by the same transformation rule, now have the sentence fragment 'old' embedded in it in front of either occurrence of 'man'. Thus, assuming the matrix and fragment each to be a paraphrase of itself (for nothing depends on this, as one can easily verify by constructing a different sentence and fragment using terms P-related to 'man' and 'old'), a P-related pair is transformed by the same rule into sentences that are not P-related: 'The old man hit the man' and 'The man hit the old man'. Similarly, by applying the same rule to 'The man hit the man' and 'The man hit the man' we can obtain both 'Who hit the man?' and 'Whom did the man hit?' Thus, also, in the case of (2), P-related sentences transform into sentences that are not P-related.

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¹ In linguistic theory the enforcement or relaxation of this restriction makes the difference between whether a rule is a transformation or a transformational schema. Cf. N. Chomsky, 'The Logical Structure of Linguistic Theory' (mimeographed, Massachusetts Institute of Technology, 1956).

A NOTE ON TELEPATHY

By PETER SWIGGART

RECENT discussion of the problem of other selves has tended to discredit the view that knowledge of another's mental state through telepathy is theoretically possible. John Wisdom argues that telepathic experiences are not what the philosopher desires 'when he asks for real knowledge of the mind of another' (*Other Minds*, p. 95). And in a comment on Wisdom's statement, I. Thalberg explains why in his opinion telepathy cannot give us such knowledge. 'Being acquainted, telepathically, with another's feelings', he writes, is nothing less than 'being in the same condition as the other person' (ANALYSIS, January 1961). Thalberg suggests that unless Bill has a cold he cannot sense what Tom senses in having a cold. If Bill feels only a nose tickle, whereas Tom has a nose tickle plus other cold symptoms, we cannot say that Bill and Tom feel the same way or that Bill can have telepathic acquaintance with Tom's sensation. 'I would say that Bill is not acquainted with Tom's tickle, because there is a crucial difference between Tom's tickle and Bill's tickle.'

Certainly the popular conception of telepathy as telephone communication *sans* telephone is not relevant to the philosopher's quest. But Thalberg's description of what telepathy must mean for the philosopher investigating the problem of knowing other minds is unsatisfactory for two reasons. First, it is evident that if Tom's nose tickle is characterized as distinct, it can be isolated from the complex of sensations with which it might be associated. And if a distinct sensation can be communicated by telepathy there is no special reason why Tom's tickle cannot be communicated to Bill. Thalberg is perhaps confusing the sense in which the quality of a sensation is conditioned by accompanying sensations with the notion of a sensation's distinct quality. His objection that a difference must exist between Tom's tickle and Bill's tickle is only the objection that a nose tickle accompanied by say a headache and a runny nose is a different sensation from a nose tickle independent of other cold symptoms. But such a difference could not prevent Bill who doesn't have a cold, but who might feel as if he did, from having a nose tickle exactly like Tom's. One might arbitrarily state, by way of defining 'sensation', that the quality of a person's sensation is uniquely influenced by his thoughts, his personality, the state of his health, etc. But this would be to beg the question of whether or not one person's sensation can be exactly communicated to another. If Tom's distinct nose tickle can be exactly given to Bill, it is irrelevant whether Tom's sensation belongs to a cold or not.

Second, even if we assume that a person's sensation cannot be identical with another's, the philosopher's question is not solved. For the possibility of telepathic acquaintance need not be restricted to instances of identical sensations. Let us suppose that Bill is healthy and in good spirits, but has a tickle in his nose. Let us further assume that Bill's tickle is by no means identical with the nose tickle of Tom, who has a nasty cold. These assumptions do not preclude the possibility that Bill's tickle is similar to Tom's and is telepathically derived from Tom. Nor does the assumption that Bill and Tom cannot have the same nose tickle prevent us from saying that through his awareness of the telepathic origin of his own nose tickle, Bill can obtain some knowledge of Tom's state of mind—knowledge unobtainable in any other way. Customary accounts of the possibility of obtaining knowledge through telepathic communication have assumed, rather strangely, I think, that such knowledge would involve identity and not mere similarity of sensation. This restriction has led to the obvious objection that it does not make sense to speak of a person's *having* another person's sensation. The recognition that telepathic communication need not involve identical sensations removes this objection. But the admission that an individual sensation might be telepathically derived does not automatically justify the claim that knowledge obtained by telepathy is in principle more reliable than knowledge obtained by conventional means. Even if Bill knew positively that his nose tickle was derived telepathically from Tom's, he would not necessarily know how similar the two nose tickles were. And if he knew that his tickle was almost identical to Tom's, he might still be in doubt as to whether Tom had a bad cold or just an allergic condition.

Perhaps the only way to solve the dispute over telepathy is to explore the difference between having a means of obtaining knowledge and actually obtaining knowledge. Knowledge obtained by telepathy might be more reliable than knowledge received through ordinary sense-experience, but the fact of its reliability would lack positive verification. Moreover, the knowledge thus obtained would tend to be trivial and unsatisfactory. Bill's nose tickle, regardless of its source, is not his best way of finding out if Tom has a cold.

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DISPOSITIONAL PROPERTIES

By J. J. C. SMART

CONSIDER the proposition that common salt (sodium chloride) is soluble in water. At a first shot, we may suggest analysing it as

If a bit of sodium chloride is immersed in water then it dissolves. (1)

Furthermore, in view of the well known difficulties in analysing the strong conditional, it is natural to construe (1) as simply

(x) (x is a bit of sodium chloride which is immersed in water $\supset x$ dissolves). (2)

(2), however, becomes vacuously true if it is the case that no bit of sodium chloride ever was, is, or would be immersed in water. Now someone might feel uneasy about this. (I am here thinking in particular of my colleague Dr. C. B. Martin, in stimulating conversations which I have had with him. However I do not wish to commit him to some view which he might wish to disavow.) Such a person might wish to say that sodium chloride would have been 'soluble in water' even in a universe in which sodium chloride never had been and never would be immersed in water. Of course a man who took the extensional point of view suggested by (2) above would no doubt regard this expression of uneasiness as illegitimate, since in order to state it one has to use the subjunctive conditional. Nevertheless I think that it is possible to accept the extensional point of view and yet give the objector much, though perhaps not all, of what he wants.

Let us imagine a universe in which sodium chloride, iron and water existed but which was such that no sodium chloride or iron ever had been or would be immersed in water. This would be in spite of the fact that in such a universe the proposition (2) above and also the proposition

(x) (x is a bit of iron which is immersed in water $\supset x$ dissolves) (3)

would be trivially true.

We can imagine that this universe contains rather clever scientists. (We may neglect the interesting question of the biochemistry of these scientists' bodies in a universe in which there were no solutions of sodium chloride in water!) By means of spectroscopic observations and the like they discover quite a lot about the quantum theory of the chemical bond. They predict that sodium and chlorine form a compound by means of the electrovalent bond. They also, from the quantum theory of the hydrogen atom and of the oxygen atom, develop the theory of

the hydrogen bond and predict the dipole moments of the water molecule. From this they predict that sodium chloride would dissolve in water. In principle there is no reason why they should not do this even though they have never observed any examples of dissolving at all (whether of sodium chloride in water or of any other solute in any solvent). Nor is there any reason why they should not deduce that the chemical bonds which account for the crystalline structure of iron are too strong to be broken on account of the dipole moments of the water molecules. Let the body of physical theory in which these deductions are carried out be called '*T*'.

We can now see a sense in which in this universe, while (2) and (3) would both be trivially true, (2) but not (3) would be true in a non-trivial way. This would be that (2), but not (3), would be deducible from the axioms of *T*. (*Cf.* H. Reichenbach, *Nomological Statements and Admissible Operations*, 1954, Definition 28 on p. 60.) This consideration may to some extent allay the anxieties of the metaphysician who wants to say that, even in the universe which we have envisaged, salt would be soluble in water and iron would not be. He might nevertheless reply that our solution only postpones the problem. Would not the axioms of *T* in effect make mention of the dispositional properties of such things as electrons? For example, to say that an electron has a negative charge is to say what it will do in relation to protons. 'But suppose there were no protons', the objector might ask, on the lines of his objection to (2) above. This time, however, the metaphysical objection seems much thinner. We need not accept the philosophy behind (or the conclusions of) Eddington's *Fundamental Theory* but we may still agree that it is probable that at the most fundamental level physics turns into cosmology. That is, the properties of the very small, namely electrons, protons, etc., are probably intimately connected with the properties of the very large, that is, the universe as a whole. We may therefore suspect that to consider the properties of an electron, in a universe in which there are no protons, is absurd in a way in which to consider the solubility of salt in water, in a universe in which no salt ever was dissolved in water, may not be so absurd.

The philosopher who gives an extensional analysis of solubility, as for example in (2) and (3), will not of course regard as legitimate any talk about what would happen in a hypothetical universe. But he can adopt the argument which I have adumbrated as an *argumentum ad hominem* against the metaphysician who believes in strong conditionals. He can say: 'Even if I accept the legitimacy of considering possible universes, which I do not, I can nevertheless give you something of what you want.'

I have, of course, chosen a particularly suitable case for my problem. It is not too far fetched to suppose that the water-solubility of sodium

chloride might have been predicted without any observations of solutions. Compare the way in which the phenomenon of the atom bomb was predicted. Some of us may wish that the proposition

(x) (x is a bit of uranium 235 of appropriate mass which is suddenly brought into contact with another such bit $\supset x$ explodes)

were true in only a trivial way. But the general topic of solubility raises some exceedingly difficult problems in physical chemistry. We may perhaps deal with some more difficult cases of solubility by recourse to some future and more perfect state of physics. Even so, such a state of affairs might never come about, owing, for example, to the complexities of calculating the perturbations of electrons in orbits. (*Not* because of any so-called 'emergent' laws, as suggested by some philosophers in the past. Compare the difficulty of predicting the position of Jupiter ten million years hence.) In fact, many of our commonest dispositional terms, such as 'fragile', raise surprisingly difficult problems in physics.

Though a person with quite a fair knowledge of physics might find it hard to explain such very simple properties as solubility and fragility, even the plain man will be willing to suppose that there is some explanation in terms of minute atomic structure. Similarly he will easily suppose some neurophysiological conditions which account for such dispositions as irascibility and vanity. W. V. Quine has exploited this, and suggests rendering ' x is soluble' as

(4) $(\exists y)(M x y \text{ and } y \text{ dissolves})$

where 'M' means 'alike in molecular structure'

(*Word and Object*, 1960, p. 224). Quine is careful to qualify this with 'in some appropriate sense'. Obviously a pretty strong sense is required. Two crystals might be, in the most usual sense, alike in molecular structure, and one dissolve where the other did not. The crystals might differ only in this: that wherever one contained a light element the other might contain a heavy element with similar valency properties. In which case, though the two crystals would be alike in molecular structure, one might be water-soluble and the other not.

An account in terms of a schema like (4) will not satisfy a person who wishes to say that something could be soluble in water even though neither it nor anything like it in molecular structure ever had been or would be put in water. In this paper I have tried to show how to give such an objector something of what he wants, without abandoning the extensional point of view save in the course of an *argumentum ad hominem*. Quine, of course, would be quite justified in refusing to give the objector anything of what he wants, since he rejects the non-extensional terms in which the question is put. Moreover he is engaged not in an analysis of language but in a rational reconstruction of it.

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RED AND GREEN ALL OVER AGAIN

By JOHN HILTON

THAT a white surface uniformly illuminated by the right balance of "red" and "green" light normally appears yellowish, rather than red and green, seems to be an accident of human physiology. With a little artificial aid even this handicap can be overcome. If the surface is looked at through a suitable arrangement of prisms a red "image" and a green "image" will be visible side by side and the surface can in this way be *seen* to be red and green all over. If that is considered cheating, a slightly more imaginary arrangement has to be made.

The auditory organs habitually sort out and enable the simultaneous appreciation of quite a number of superimposed frequencies. It might however happen that D flat on a bassoon and F sharp on an oboe played together sounded like E on a 'cello, though I have not yet heard of a case of such a thing's happening. Contrariwise, there is no evident absurdity in supposing a mutation in the visual system enabling red and green light to be sorted out and a surface seen as red and green all over. Indeed, although it is a little difficult to be sure that one can imagine what the effect would be like for a large uniform patch regarded passively, something approaching the likely effect is reached, in a more active and sophisticated way, when we "see" a distinction between atmospheric and local colour, "see" a smear of green glaze crossing several different juxtaposed ground colours or "see" Mr. Pears's green bus reflected in the red one.¹ (The inverted commas are to distinguish this more active and sophisticated seeing from passive regarding.)

Is anything self-evident except that if a surface is only emitting light of one wave-length it cannot be emitting light of another wave-length or that if a person is seeing an area as only red he is not seeing it as any other colour?

And is not the situation much the same in other cases of pairs of, at first sight, incompatible qualities mentioned by Mr. Peter Remnant?² In the case of hot and cold is anything self-evident except that if the mercury or other thermometric medium is at one point on the scale it cannot at the same time be on another point on the same scale? Berkeley remarked that the same water could simultaneously feel hot to one hand and cold to the other. If anyone feels that the two hands are so far apart as scarcely to count as belonging to one observer he might try chilling half one hand and warming the other half or even selectively treating different nerve-endings.

¹ D. F. Pears, 'Incompatibilities of Colours': Logic and Language, 2nd Series, ed. A. Flew: Blackwell, 1953.

² 'Red and Green All Over Again,' ANALYSIS 21.4, 1961,

As to bland and aromatic, Mr. Remnant must be a devotee of the very plainest cooking. He may be inclined to say that different ingredients in the sauce are responsible when these different effects are discriminated, but this is from his culinary knowledge; if the sauce is well mixed all parts of it have the same range of blended but distinguishable tastes.

I cannot at the moment think of any self-evidently incompatible qualities unless, either, measurement is involved (e.g. different temperatures), or, there is a logical contradiction (e.g. a surface cannot be perfectly black and coloured, or white, because there cannot be no light and light).

Part of Mr. Pears's discussion¹ was about whether it is self-evident that red is different from green. The only sense I can give to this question is whether it is self-evident that light of a wave-length that normally looks green to normal people could not look red to someone with a freak physiology. This is not self-evident to me. I know that even my own two eyes see colours slightly differently and it is no great step to imagine this difference carried further. But this can hardly be the point.

I feel less puzzled about the compatibility of red and green than about the question of whether colour and extendedness are indisseverable. Would it be possible—say by some interchange of optical and auricular nerves—to “hear” air vibrations as unextended colours and to “see” electro-magnetic vibrations as extended sounds? If not, why not?

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¹ *Op. cit.*

Contents of ANALYSIS for January include: ‘Some Questions for Miss Anscombe about Intention’ by David Braybrooke and others, together with a reply by Miss Anscombe; ‘Are Inductive Generalisations Quantifiable?’ by John O. Nelson; ‘On a Supposed Tautology’ by Lennart Aqvist; and ‘Virtue: an Analysis and a Speculation’ by Jon Wheatley.

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